factors for yellow endosperm, Y_1 and Y_2, of which one is much more effective in producing the yellow pigment than the other. Such an assumption he regards as "violent," it being just as violent as have been the assumptions of all Mendelian experimentalists who have made mathematical interpretations of breeding facts. The author is also greatly disturbed over the question of whether or not the segregation ratios that he obtained fit the theory of error. It seems to the reviewer, however, that considering the possibility of experimental error in work with maize, he is to be congratulated on having done some very careful work to have them fit "theory" as well as they do.

Notwithstanding the fact that the results obtained agree well with the assumption of two yellow factors that are given above, with only a few minor variations of classification due to the difficulty of distinguishing light yellows from white, the author concludes "that while the segregation is usually numerically exact, it is by no means complete; that is, the dominant character is not completely absent from individuals of the recessive class." "This," he says, "is shown not only by the presence of a faint yellow color in most of the seeds, but also by the fact that apparently pure white seeds from an ear in which the classes were well marked may produce seed with a fully developed yellow color when self-pollinated." Consequently he favors the idea of gametic impurity in the sense that extracted dominants and recessives may transmit traces of the alternative character.

Again this conclusion seems opposed to the facts submitted. If one has a set of light yellow and white seeds in an apparent ratio of 3:1, he makes his classification as best he may by somatic appearance. He then grows the whole series and finds out *what the true classification of the parents was.* This the reviewer has done on similar material, with the result that the ratio of the mother seeds *proved* to be 3:1; this the author either has not done or has not reported. If then the *white ears* obtained do not again breed true, one might have the right to assume gametic contamination; but the author reports no such evidence. As a matter of fact, extracted recessives and extracted dominants do appear to throw the alternative character on rare occasions, but the phenomenon is so rare that one may better assume that a germinal rearrangement (mutation) has occurred. Of course in any species some variations are more likely to occur than others, which may be taken as evidence of a kind of latency. But this is only the kind of latency that is analogous to the tendency of a chlorine atom to split off from a complex benzene derivative, rather than one of the more conservative radicals such as methyl. It is evidence that certain rearrangements in a particular germ plasm are more likely to occur than others.—E. M. EAST.

Studies of Nicotiana hybrids.—In two papers, appearing almost simultaneously, GOODSPEED[8] has reported the results of his investigations on

[8] GOODSPEED, T. H., Quantitative studies of inheritance in *Nicotiana* hybrids. Univ. Calif. Publ. **5**: no. 2. pp. 87–168. *pls. 29–34.* 1912; *ibid.* no. 3. pp. 169–188. 1913.

possible correlations between seed characters and plant characters, and on the inheritance of certain quantitative character complexes in crosses between various *Nicotiana* species and varieties.

In that part of the contribution concerned with somatic correlations the author deals with a cross between the varieties *virginica* and *macrophylla* of the species *Nicotiana tabacum*. F_1 seeds were divided arbitrarily into the classes light, medium, and heavy. The light and medium seeds germinated more quickly than the heavy seeds, and plants resulting from the former matured more quickly than those from the latter. It does not appear, however, that the general belief that heavy seed gives more vigorous plants than light seed is incorrect. Moreover, the total germination of heavy seed was higher than that of the other two classes.

The author continually speaks of the dominance of one plant over another, a mediaeval mode of expression that makes it impossible to draw any conclusions from his observations on his F_1 generation. Likewise he finds that the heavy seed gave, in F_2, 39 per cent of "dominants" (resembling *macrophylla*), 52 per cent of intermediates, and 9 per cent of "recessives" (resembling *virginica*); while the light seed gave 18 per cent dominants, 49 per cent of intermediates, and 33 per cent of recessives. With such a method of describing results it is not surprising that he invents a theory for their interpretation that will no doubt be very interesting to cytologists, for in it he assumes that the "tube nucleus" of the pollen grain unites with the fusion "endosperm" nucleus of the embryo sac. He assumes that there are two "determiners," one functioning to produce *virginica* characters and the other *macrophylla* characters. The generative nucleus, he says, may bear either, and the "tube nucleus" may bear either. The same alternatives are assumed for the egg nucleus and the fusion "endosperm" nucleus. Then, simply by having a *macrophylla* generative nucleus unite with a *macrophylla* egg nucleus, and a *macrophylla* "tube nucleus" unite with a *macrophylla* fusion nucleus, he gets a heavy seed having *macrophylla* characters. *Quod erat demonstrandum.* Charitably granting that the words "tube nucleus" were slips of the pen, there is no excuse for founding a theory that the two male nuclei carry different "determiners" upon unsupported data of this character.

In the second part of his work, the author has studied the degree of dominance and the variation in size of corolla diameter in the parents and F_1 generation of crosses between varieties of *Nicotiana acuminata*. The corolla breadth of the F_1 generation was found to be the arithmetical mean of the two parents. The fluctuation in corolla breadth, both in individual plants and in the population as a whole, was greater in F_1 than in the parents. These conclusions, at least as to the degree of variability of the F_1 generation, are at variance with the results of several careful investigators (the reviewer can count twelve such offhand), but it is impossible to criticize GOODSPEED's data, for he does not give them in the form of frequency distributions. He simply reports maximum and minimum measurements, which may or may not mean anything.

He does indeed give two plates of frequency polygons, but his distributions are for number of flowers measured on particular dates, with no statement as to their size, and for relative frequency of flowers of certain sizes, with no data on the actual number of flowers measured or the number of plants upon which they were borne. Apparently the parents upon which data were taken were too few to warrant such sweeping conclusions.

In the second paper, also, one gathers that the F_2 generation there reported on is more variable than the F_1 generation; but no data are recorded. This paper purports only to be a note, however, and one may expect some data of greater consequence when the really large amount of work that the writer has done is reported in full.—E. M. EAST.

Knot disease of citrus trees.—HEDGES and TENNY[9] give a complete account of a knot disease of citrus trees that had been briefly described in a preliminary account by Miss HEDGES.[10] The disease has been found on lime trees in Jamaica and in one instance in Florida. It manifests itself by woody knots or swellings which appear on the branches and trunks of the diseased trees. The knots are usually round or somewhat elongated in the direction of the axis of the branch which bears them. They attain a diameter of 2–3 inches, and by their growth usually girdle the branch upon which they are seated, this causing the death of all the parts of the branch above the knot. Groups of fascicled branches, forming witches-brooms, often grow out from the knots, but these branches also are short-lived. The knots consist mostly of woody tissue, at first covered by bark which soon dies and crumbles away. All the tissues of the knots, as well as the tissues of the branches near the knots, are found to be infected with the brown mycelium of a fungus which was described by Miss HEDGES as *Sphaeropsis tumefaciens*. The mycelium of this parasite has been observed to spread to a distance of 45 cm., and it seems probable that it can spread to greater distances. Secondary knots are produced by the mycelium which spreads through the branches. The growth of the fungus on a large number of media, its characteristics, and numerous infection experiments are described at length by the authors.—H. HASSELBRING.

The cause of leaf asymmetry.—BOSHART, working in GOEBEL'S laboratory, reports the results of certain observations and experiments on asymmetry and anisophylly.[11] He concludes that the size of any given leaf part is determined by the area it occupies in the vegetative point. Further development

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[9] HEDGES, F., and TENNY, L. S., A knot of citrus trees caused by *Sphaeropsis tumefaciens*. Bur. Pl. Ind. Bull. 247. pp. 9–74. *pls. 10. figs. 8.* 1912.

[10] HEDGES, FLORENCE, *Sphaeropsis tumefaciens*, nov. sp., the cause of the lime and orange knot. Phytopath. **1**:63–65. *pl. 1.* 1911.

[11] BOSHART, K., Beiträge zur Kenntnis der Blattasymmetrie und Exotrophie. Flora **103**:91–124. 1911.